

**Comments on *Cognettia* Nielsen & Christensen, 1959 (Annelida, Oligochaeta, ENCHYTRAEIDAE): proposed precedence over *Euenchytraeus* Bretscher, 1906 and *Chamaedrillus* Friend, 1913**

(Case 3689; see BZN 72: 186–192)

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As we were responsible for re-establishing *Euenchytraeus* Bretscher, 1906 and *Chamaedrillus* Friend, 1913, suggesting that *Cognettia* Nielsen & Christensen, 1959 should be treated as a junior synonym of *Chamaedrillus* (Martinsson et al., 2014; cited as 2015a by Schmelz et al.), we would like to give some supplementary information on the case, and also explain the reasoning behind the reestablishments. In their appeal to the Commission, Schmelz et al. (BZN 72: 186–192) give a good repetition of the taxonomical history of the taxa involved (detailed by us in Rota et al., 2008; Martinsson et al., 2014, 2015). However, we would like to highlight a few additional points.

1. The case in question (our invalidation of *Cognettia* in favour of *Chamaedrillus* and *Euenchytraeus*) arose after two of us (Martinsson & Erséus, 2014) provided molecular evidence that *Cognettia sphagnetorum* (Vejdovský, 1878) in its commonly accepted definition, based on Nielsen & Christensen's (1959) revisionary work, is a non-monophyletic complex of species. Given the relevance of these worms in soil ecological studies, this resulted in an urgent need of taxonomic revision within *Cognettia*, a revision that we carried out (Martinsson et al., 2014) using genetic and morphological data.

2. Before our revision (Martinsson et al., 2014), *Pachydrillus sphagnetorum* Vejdovský, 1878, the nominal species designated by Nielsen & Christensen (1959) as type species of *Cognettia*, had no extant type material.

3. The type species of *Chamaedrillus*, *Ch. chlorophilus* Friend, 1913, is represented by two syntypes preserved in the Natural History Museum, London (NHM), slide BMNH 1949.3.1.32 (Martinsson et al., 2014). As documented by register records at NHM, this slide contains 'Friend's types of *Chamaedrillus chlorophilus*' (source: NHM (2014). Dataset: Collection specimens. Resource: Specimens, <http://dx.doi.org/10.5519/0002965>, Retrieved: October 2015). We borrowed this slide for our

re-investigation of *Ch. chlorophilus* (Martinsson et al., 2014 and designated the mature syntype as lectotype).

4. When Nielsen & Christensen (1959) redescribed the type species of *Cognettia*, they mentioned the frequent occurrence of a 'single sub-median supernumerary bulb' in front of the penial bulbs. In revising the *C. sphagnetorum* species complex, we found this feature to occur only in combination with the chaetal arrangement (2 chaetae in preclitellar lateral bundles and 3 chaetae in other bundles) that is specific for *Ch. chlorophilus* (Martinsson et al., 2014). Thus, Nielsen & Christensen (1959) had – intentionally or not – treated *C. sphagnetorum* and *Ch. chlorophilus* as one and the same species. We do not know whether Nielsen & Christensen (1959) missed that *Ch. chlorophilus* had been synonymised with *C. sphagnetorum* by Černosvitov (1937a) (subjective junior synonymy) – which would be noteworthy, as they discussed Friend's taxa in their introduction. In any case, Nielsen & Christensen (1959) established *Cognettia* on the same type species as that of *Chamaedrillus* and this indeed makes *Cognettia* as a genus an invalid name (objective junior synonymy). It is as if Nielsen & Christensen (1959) had replaced *Chamaedrillus* by an unjustified emendation and without proposing *Cognettia* expressly as a new replacement name (nomen novum). *Chamaedrillus* was still an available name, its replacement not being required by any provision of the Code.

5. According to Schmelz et al. (2015), the main reason for giving precedence to *Cognettia* over *Chamaedrillus* would be the prevention of nomenclatural confusion in view of the importance of *Cognettia* species, and particularly of *C. sphagnetorum*, in soil ecological research. The problem is, however, that neither *C. sphagnetorum* nor *C. glandulosa* (Michaelsen, 1888b), the two most 'popular' enchytraeid names in soil ecology literature, each correspond to single species when identified, as has been done for decades, using Nielsen & Christensen's (1959) diagnoses – or even worse, if identified according to the broadened definitions recently proposed by Schmelz & Collado (2010, 2012a). It is still the opinion of several ecologists that the ENCHYTRAEIDAE or at least their genera respond homogeneously to environmental drivers. However, community analyses conducted at the species level (e.g. Rota et al., 2013) show clearly that within enchytraeid genera there can be a variety of ecological tendencies. Nielsen & Christensen's (1959) diagnosis of *C. sphagnetorum* identified a non-monophyletic set of species (Martinsson & Erséus, 2014) that has now been formally resolved into four separate *Chamaedrillus* species with distinct ecology (Martinsson et al., 2014). *Cognettia glandulosa*, as well, in its long-accepted definition included two well-separate genetic lineages (Martinsson & Erséus, 2014); these too have been recently formalized as two distinct *Chamaedrillus* taxa, morphologically very similar (differing mainly in size) but preferring different habitats (Martinsson et al., 2015). Unfortunately, when reading through the ecological literature on *Cognettia*, one cannot tell which single taxonomic unit was the object of each ecological study, or where and when a mixture of species was involved.

For the reasons stated above, neither the bulk of ecological studies nor the recent soil biology textbooks will be unburdened from serious ambiguities if the name *Cognettia* is preserved, because data are referred monospecifically to species assemblages with heterogeneous ecology. It is our hopeful conviction, instead, that a new nomenclature combined with a better taxonomic resolution at species level will serve

to prompt a fresh start in the ecological characterization of the individual taxonomic units in the '*Cognettia*' world.

6. Taxonomic clarity, geographic distribution and ecological range of genera and species are all aspects still in a state of uncertainty for many components of the ENCHYTRAEIDAE (Rota & de Jong, 2015) and all efforts should be focused to improve quality in genera and species circumscriptions, for instance as fundamental prerequisites for biodiversity assessment in any geographical region. The nomenclatural distinction of *Euenchytraeus* Bretscher, 1906 from *Chamaedrillus* aims at this same purpose, as we already have evidence that the two taxa are phylogenetically separate.

7. When we started our revision (Martinsson et al., 2014) and considered that *Chamaedrillus* and *Euenchytraeus* should replace the widely used *Cognettia*, we were at first concerned that changes in customary usage might create instability, and reflected on submitting to the Commission a case similar to the one here discussed. However, personal experience from one of us (Erséus et al., 2005; ICZN, 2007) made us hesitate, as it is a fairly long process with an uncertain outcome. On the other hand, the synonymy between *Cognettia*, *Chamaedrillus* and *Euenchytraeus* was pointed out earlier by Schmelz & Collado (2010, p. 82) themselves, with the remark: "To conserve *Cognettia* as valid name a proposal towards the ICZN is necessary", but such a proposal (that we would have welcomed, if it had been submitted to the Commission earlier) never appeared in the next few years. Therefore, having underway our phylogenetic revision of these taxa, we decided to simply follow the code: re-establish *Chamaedrillus* as a senior synonym of *Cognettia* and resuscitate *Euenchytraeus* as a valid genus (Martinsson et al., 2014).

8. The taxonomy of ENCHYTRAEIDAE has for a long time been under soft rules and some of the family's early nomenclature is in continuous revision and evolution. It is true, as Schmelz et al. (2015) state, that 'the nomenclatural and taxonomic framework established in Nielsen & Christensen (1959) was widely accepted by taxonomists and non-taxonomists' and that 'their 1959 monograph, followed by two supplements (Nielsen & Christensen, 1961, 1963) launched a new era of research with enchytraeids, particularly in the field of soil ecology'. However, Nielsen & Christensen (1959, 1961, 1963), as often noted (e.g. Brinkhurst, 1971; Schmelz, 2003) and by their own admission (1959, p. 10), did not always comply with the rules of the Code, as they 'decided not to embark upon an unpromising formal revision of all original descriptions and type material that might be extant in various collections'. Thus, in spite of their huge effort and merits, their critical revision of ENCHYTRAEIDAE left the nomenclature of many taxa subject to debate and the genus-level and species-level taxonomy in many cases problematic and typological. By recognizing and delimiting monophyletic groups based on common descent, phylogenetic methods are gradually alleviating some of the classification problems – as are molecular analyses effective in helping detecting and separating cryptic species. With regard to nomenclature, Rota & de Jong (2015) have listed the novelties (new nomina and nomenclatural acts) occurred just in the last 10 years among the enchytraeid species and genera living in Europe: 39 new species, 11 new combinations, 8 reinstatements as valid names, 4 rejected synonymies.

9. The name change from *Cognettia* to *Chamaedrillus* has already started to be accepted by both ecologists and taxonomists and, since the time of our revision (Martinsson et al., 2014), several publications have treated *Chamaedrillus* as a valid

name (Boros & Dózsa-Farkas, 2015; Dózsa-Farkas et al., 2015; Holmstrup et al., 2015; Martinsson et al., 2015; Rota & de Jong, 2015; Schmelz & Collado, 2015; Torii, 2015), indicating that the name change from *Cognettia* to *Chamaedrillus* may not create the feared instability predicted by Schmelz et al. (2015). Furthermore, two species, *Chamaedrillus varisetosus* Martinsson, Rota & Erséus, 2015 and *Chamaedrillus ozensis* Torii, 2015, have been described after our revision. There is a risk that it would be even more confusing and create more instability if the name for this genus is to change back to *Cognettia* from *Chamaedrillus*.

In fact, more confusion in zoological nomenclature may originate from, and be sustained by, inconsistent, contradictory communications: for instance, Schmelz & Collado (2015) in updating the list of valid taxa of ENCHYTRAEIDAE, accepted the new *Chamaedrillus* and *Euenchytraeus* synonymies, but then, shortly afterwards, the same authors proposed rejection in favour of the old *Cognettia* names (Schmelz et al., 2015). Similarly, at the species level, those same authors (Schmelz & Collado, 2010, p. 79; 2012a, p. 56; 2012b, p. 70) first lumped a number of morphologically distinct species under the name '*C. sphagnetorum* sensu lato', but then reconsidered their act by publishing again those species as revalidated and assigned to *Chamaedrillus*.

10. In sum, the progress in the knowledge of the family ENCHYTRAEIDAE can be furthered only by a continually refined taxonomy and by nomenclature following the system: stability must not have priority over lack of ambiguity. We are well aware of the multidisciplinary relevance of zoological names, and it is precisely for this reason that names should be assigned correctly and univocally, and be used consistently by taxonomists, ecologists and experimental scientists as means for scientific communication. Through our proposed new nomenclature, and improved taxonomic resolution, we will gain a much better understanding of several '*Cognettia*' taxa that were thus far confounded in soil research.

11. The points above lead us to the conclusion that *Euenchytraeus* Bretscher, 1906 and *Chamaedrillus* Friend, 1913 ought to be kept as valid names, and that there are no contraindications for treating *Cognettia* Nielsen & Christensen, 1959 as a junior synonym of *Chamaedrillus*.

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We support the proposition to give precedence of the widely used name *Cognettia* over *Chamaedrillus* and *Euenchytraeus*, which had not been used as valid for almost a century. In addition to the detailed explanations by Schmelz et al. (2015) we emphasize the importance of the name in long-term soil observation programmes (soil monitoring) where it is used to characterize specific soil communities with *Cognettia sphagnetorum* as indicator species. In this context the terms ‘Cognettietalia’ (Order), ‘Achaeto-Cognettion’ (Alliance), ‘Cognettion sphagnetorum’ (Alliance), ‘Achaeto-Cognettietum’ (Association) and ‘Cognettietum sphagnetorum’ (Association) are established designations within the system of decomposer community types (Graefe, 1993a, 1993b; Fründ & Graefe, 1994; Beylich et al., 1994, 1995; Graefe, 1998; Graefe et al., 2001, 2002; Höper, 2002; Beylich & Graefe, 2002; Graefe & Beylich, 2003; Beylich et al., 2005; Beylich & Graefe, 2009). These community types are also reported in more than 50 unpublished technical reports on behalf of soil protection agencies. The re-establishment of a generic name that has been out of use for generations for such a well-known and widespread species as *Cognettia sphagnetorum* will in our opinion cause considerable confusion instead of clarifying the situation.

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The nominal genus *Cognettia* has been used during the last 55 years in hundreds of books and papers in different branches of biology. Replacing it with either of two virtually forgotten names will cause unjustified mess not only in taxonomical but also in applied literature. Even more, the discussed synonymy between these two names, *Chamaedrillus* and *Euenchytraeus*, will produce endless nomenclatural instability. That is why I support conservation of the genus name *Cognettia*, suppressing the priority of the senior synonyms *Chamaedrillus* and *Euenchytraeus*.

**Comment on *Tipula contaminata* Linnaeus, 1758 (currently *Ptychoptera contaminata*; Insecta, Diptera): proposed conservation of prevailing usage through designation of a neotype**

(Case 3664; see BZN 71: 237–243)

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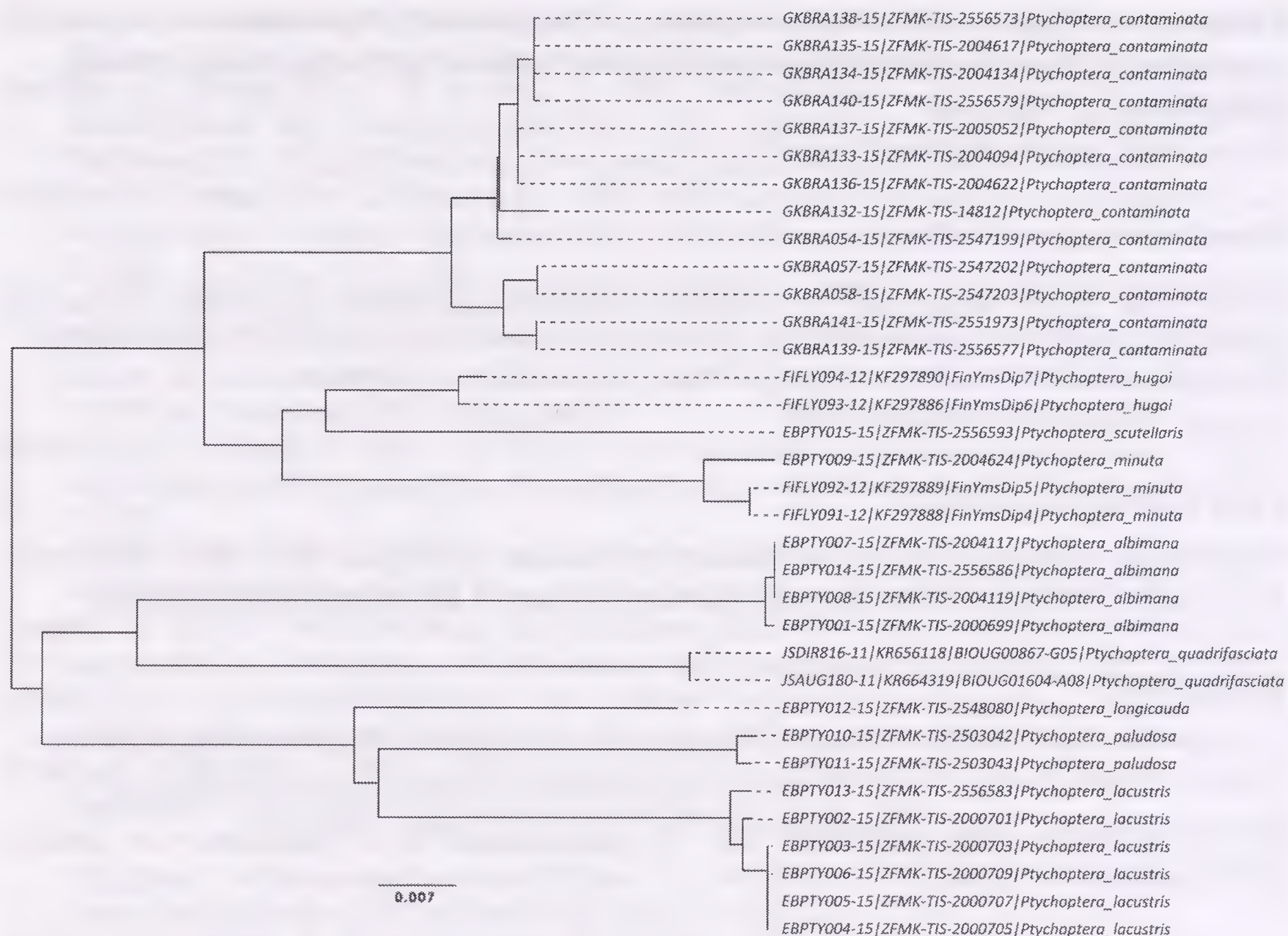
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In their recent revision of the holotype of *Tipula contaminata* Linnaeus, 1758, Fasbender & Courtney (BZN 71: 237–243) found the specimen to belong to TIPULIDAE and not to match the current concept of the species as near-universally accepted from Meigen (1803) onwards. Retaining Linnaeus' original syntype as valid would threaten not only the species nomen, but also the stability of the genus name *Ptychoptera* Meigen, 1803 and the family-group name PTYCHOPTERIDAE Osten Sacken, 1862, both well-known and well-established nomina in dipteran taxonomy. Their proposed solution to the problem was to designate a Danish male specimen as neotype of the species, to preserve the concept of *Ptychoptera contaminata* as recognized by at least 22 different authors. We essentially agree with these interpretations and consider their proposal an elegant solution to the problem.

Designating a neotype for a well-established species can be problematic if the taxonomic status of the species is questionable, e.g. if it forms part of a species complex. Luckily, the available evidence suggests that *Ptychoptera contaminata* is a valid species well delimited from other species of *Ptychoptera* in the Palearctic region.

In our ongoing work on DNA barcoding of European PTYCHOPTERIDAE, *Ptychoptera contaminata* sequences form a distinct cluster separate from all other species (Fig. 1). The molecular data set is available online at BOLD (<http://dx.doi.org/10.5883/DS-EBPTYCH>). In our analyses, sequence variability is low, with a maximum intraspecific p-distance of 1.61% (Fig.1). Furthermore, we are not aware of any morphological variation in *P. contaminata* to suggest any ambiguity in the concept outlined in Case 3664.

In conclusion, we find Fasbender & Courtney's argument compelling and recommend that the Commission follow both of their recommendations.



**Fig. 1.** Unrooted neighbour-joining phenogram of *Ptychoptera* CO1 sequences in GBOL and NorBOL, computed based on pairwise distances in MEGA6 (Tamura et al., 2013). BOLD accession numbers are given for each specimen.

## Acknowledgements

DNA barcodes of European PTYCHOPTERIDAE are obtained through NorBOL, which is funded by the Norwegian Research Council and the Norwegian Biodiversity Information Centre; and through GBOL, which is funded by the German Federal Ministry for Education and Research (BMBF #01LI1101A).

## Additional references

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**Comments on the work ‘The White-cheeked Geese: *Branta canadensis*, *B. maxima*, *B. ‘lawrensis’*, *B. hutchinsii*, *B. leucopareia*, and *B. minima*. Taxonomy, ecophysiographic relationships, biogeography, and evolutionary considerations, Volume 1, Eastern taxa; Volume 2, Western taxa; biogeography, and evolutionary considerations’ by Harold C. Hanson: proposed suppression for nomenclatural purposes**

(Case 3682; see BZN 72: 209–216)

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As a member of the Working Group on Avian Nomenclature of the International Ornithologists’ Union I contributed an opinion on the draft of the proposal by Banks, LeCroy & Schodde and supported their application.

As Managing Editor of the 4th Edition of the Howard & Moore Complete Checklist of the Birds of the World, of which the non-passerine Volume appeared in 2014 co-edited with J.V. Remsen Jr., consideration was given to the Hanson monograph. At our request our friend Norbert Bahr listed the new taxa proposed in Hanson’s book. The three line introduction to that list (pp. 394–397 in the Checklist) stated that the Volumes awaited assessment by the American Ornithologists’ Union’s Committee on Classification and Nomenclature. The checklist editors decided to prevaricate in this way because they could not seriously accept Hanson’s proposals, but nor did they feel that authority to reject them in whole or in part lay elsewhere than with the competent North American ornithological community. We were already aware that the United States Fish and Wildlife Service considered Hanson’s work to be a major potential destabilisation of the debate about species limits in this complex and that many of the subspecific proposals were based on specimens collected in their winter quarters without knowledge of their home range and thus had the potential to create serious confusion.

I have no hesitation in recommending that the Commission consider the proposed suppression of this work to be both appropriate and justified.

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I write to strongly support the requests stated in Case 3682 to suppress this work by Hanson. Banks et al. have given a compelling detailed description, explanation and rationale, which justify this critical action based on an objective and reasonable assessment of your defining and guiding Code.

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We are writing to register our support for the proposed suppression of the work 'The White-cheeked Geese. . .' by Harold C. Hanson. Suppression of entire taxonomic works for nomenclatural purposes should be considered only in exceptional cases. We believe this is such a case. The aforementioned work introduces an extreme number of species-group names on the basis of inappropriately chosen types and inadequate diagnoses. These and other issues identified by Banks, LeCroy & Schodde (BZN 72: 209–216) would cause an unacceptable number of problems for ornithological nomenclature, which are best avoided by suppressing the entire two-volume work and placing it on the Official Index of Rejected and Invalid Works in Zoological Nomenclature.

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I am in complete agreement with Banks, LeCroy and Schodde that the works by Hanson on white-cheeked geese (*Branta*) should be suppressed for purposes of nomenclature. As the authors have abundantly demonstrated, the works were undertaken with little or no regard or knowledge of proper nomenclatural procedures. It might also be noted that failure to suppress this work might actually discourage future researchers from undertaking the studies still needed to determine the extent and geographical limits of variation within this complex group, as no one would wish to be saddled with the unrewarding task of trying to relate their legitimate results to these confusing accounts by Hanson.

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I strongly support the suppression of the work by Harold C. Hanson (2006–2007), as proposed by Banks, LeCroy & Schodde. The comprehensive work done by these authors clearly shows that, if not suppressed, Hanson's work will create both nomenclatorial and taxonomic confusion and eventual chaos among those dealing with the taxa involved. Furthermore, I do not believe that any of the other options discussed by Banks, LeCroy & Schodde in their paragraph 11, other than complete nomenclatorial suppression of the entire work, will succeed in solving the great number of irregularities and flaws contained in Hanson's work.

Letters of support for Case 3682 were also received from Daniel D. Gibson, (*University of Alaska Museum, Fairbanks, Alaska, U.S.A.*; e-mail: avesalaska@gmail.com), Bruce M. Beehler (*Division of Birds, MRC 116, National Museum of Natural History, PO Box 37012, Smithsonian Institution, Washington DC, U.S.A. 20013*; e-mail: bruceembeehler@gmail.com) and Jay M. Sheppard (*Ornithological Literature, Laurel, Maryland, U.S.A.*; e-mail: jaymsheppard95@gmail.com).

**Comment on the proposed conservation of *Neobisium* Chamberlin, 1930, NEOBISIOIDEA Chamberlin, 1930, NEOBISIIDAE Chamberlin, 1930 and NEOBISIINAE Chamberlin, 1930 (Arachnida, Pseudoscorpiones, Chelonethi) by designation of *Obisium muscorum* Leach, 1817 as the type species of *Obisium* Leach, 1814** (Case 3616; see BZN 70: 75–81, 249; 72: 221–226)

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The present comment addresses Alonso-Zarazaga's criticisms concerning Case 3616 and the alternative proposals he presents to the Commission for its resolution (BZN 72: 221–222). According to Alonso-Zarazaga, my application was flawed because it failed to explain the 'real' situation in Leach's (1814) text, particularly the fact that *Obisium* Illiger, 1798 was treated as a synonym of *Chelifer* Geoffroy, 1762 in that work. Unfortunately, Alonso-Zarazaga does not take into account my earlier analysis of this matter (Judson, 2012), with the result that his presentation is an oversimplification. The relevant synonymies can be summarized as follows:

1. Geoffroy (1762) published the generic name *Chelifer* for *Acarus cancroides* Linnaeus, 1758 (a pseudoscorpion) and *A. longicornis* Linnaeus, 1758 (a mite), without designating a type species. The name *Chelifer* was invalid because it was published in a work that was not consistently binominal (Article 11.4). However, it was later conserved by Opinion 1542 (BZN 46: 143).

2. Illiger in Kugelann & Illiger (1798) introduced the name *Obisium* Illiger, 1798 for '*Scorpio cancroides* et *cimicoïdes* Fabr.' [i.e. *Acarus cancroides* Linnaeus, 1758 and *Scorpio cimicoïdes* Fabricius, 1793] in a simple list of taxa. No diagnosis was

provided, no type species was designated and no mention was made of the genus *Chelifer*.

3. The entry ‘Crustaceology’ of Brewster’s (1814) *Edinburgh Encyclopædia* treated three species of pseudoscorpions, all of which are assigned to the genus *Chelifer*. Within the genus, two groups of species were diagnosed: the first (“\*”) included *C. cancroides* and *C. cimicoides*; the second (“\*\*”) contained only *C. trombidoides* Latreille, 1804 (incorrectly spelt ‘*trombidoides*’, but correctly spelt *trombidoides* in the synonymy given). The name *Obisium* is used in two different ways in this article:

(i) The editor, D. Brewster, treated ‘*Obisium* Illiger, Walckenaer’ as a synonym of ‘*Chelifer* Geoff. De Geer, Oliv. Lam. Herm. Latreille’. This is clear from the ‘Obs.’ he inserted at the end of the section on pseudoscorpions, which reads ‘These two divisions of the genus certainly have distinct characters enough to form two genera; we therefore, perhaps, should follow Mr Leach, who proposes to call the first division *Chelifer*, a name first given by Geoffroy; the second *Obsium*, a name proposed by Illiger for the genus as it now stands.’

(ii) Leach treated *Obisium* as a monotypic genus, containing only the species *Obisium trombidoides*. Leach did not give any indication of authorship for *Obisium*; the attributions of the name to Illiger in Leach (1814) are those of Brewster.

While I accept that the status of *Obisium* sensu Leach is open to different interpretations, due to the ambiguous way in which it was introduced, it seems to me that Alonso-Zarazaga errs in ignoring Brewster’s intervention in Leach (1814). Alonso-Zarazaga’s comment that ‘The name ‘*Obisium trombidoides*, Leach’s MSS.’ appears in the synonymy of [*Chelifer trombidoides*] and is not used as a valid name’ is correct, but that does not mean that *Obisium* Leach, 1814 must be considered unavailable. Article 11.6.1 allows *Obisium* Leach, 1814 to be considered available because it was treated as an available name and adopted as the name of a genus before 1961. Alonso-Zarazaga’s comment that ‘Sundevall (1833) and Harvey & Mahnert (2011), among others, were right in their treatments of *Obisium* sensu Leach as a later use of *Obisium* Illiger’ is largely irrelevant because no author interpreted the group in the same sense as Leach (i.e. as a genus including *Chelifer trombidoides*) between 1843 and 2012 (see paragraphs 4 and 9 of the original application). As for Sundevall (1833), he listed ‘*Chelifer*’ and ‘*Obisium*’ as separate genera, so he was clearly using *Obisium* in Leach’s sense, even if he attributed it to ‘Ill. Leach, Herm.’ (Judson, 2012). It is perhaps worth repeating that the lists of authors appended to generic names in the early literature are more like abbreviated bibliographies than a statement of authorship in the modern sense of the Code.

Alonso-Zarazaga’s comments about generic concepts modified through the addition or removal of species being misapplications (his ‘misidentifications’) are obviously correct, but these can only apply to later uses that do not form part of the application, since he considers Leach (1814) to have treated *Obisium* as a synonym of *Chelifer*. His interpretation is at odds with his own treatment of at least one analogous case, namely that of *Bryaxis* Kugelann, 1794 and *Bryaxis* Leach, 1817, evoked by Judson (2012). Coleopterists, including Alonso-Zarazaga (Bouchard et al., 2011, p. 185), and the Commission (Opinion 887; BZN 26: 133) have accepted *Bryaxis* Leach, 1817 as an available homonym, as opposed to a misapplication, even though Leach (1817) did not present it as a new name and was aware that it had been used before for another genus of PSELAPHINAE.

Although Alonso-Zarazaga's alternative proposals are welcome insofar as they would attain the same end, it is evident that they are largely inconsistent with his own interpretation of the case. If, as he believes, *Obisium* Leach had simply never existed, most of his proposals would be superfluous. Without a genus *Obisium* Leach, there could not be an overlooked fixation of *Chelifer trombidoides* as its type species, hence there would be no question of a synonymy between *Chthonius* and *Obisium*. Moreover, the family-group name OBISIIDAE Sundevall, 1833 would either be unavailable or an objective synonym of CHELIFERIDAE Risso [1827] and Simon's (1879) designation of a type species for *Obisium* Leach would be meaningless. Consequently, the only problems requiring action by the Commission would be Chamberlin's (1930) proposals of NEOBISIIDAE, NEOBISIINAE and *Neobisium* as replacement names for OBISIIDAE Sundevall, 1833, OBISIINAE Sundevall, 1833 and *Obisium* Leach, respectively, and his citation of Simon (1879) for the designation of *O. muscorum* as the type species of *Obisium* (and hence of its replacement, *Neobisium*). Thus, it would suffice that the Commission rule that NEOBISIIDAE, NEOBISIINAE and *Neobisium* were proposed as new taxa by Chamberlin (1930), and not as replacement names, and that the type species of *Neobisium* was designated as *Obisium muscorum* Leach, 1817 by Chamberlin (1930). These actions would assure the conservation of the names *Neobisium*, NEOBISIOIDEA, NEOBISIIDAE and NEOBISIINAE, but they would have little to do with the historical reality. I therefore maintain that the proposals in the original application represent the most suitable means of conserving these names.

#### **Additional reference**

- Bouchard, P., Bousquet, Y., Davies, A.E., Alonso-Zarazaga, M.A., Lawrence, J.F., Lyal, C.H.C., Newton, A.F., Reid, C.A.M., Schmitt, M., Ślipiński, S.A. & Smith, A.B.T. 2011. Family-group names in Coleoptera (Insecta). *ZooKeys*, **88**: 1–972.